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ABSTRACT

Several approaches to the study of play are examined and critiqued, including Matthews and Matthews' (1982) paradigm case approach and Sutton-Smith and Kelly-Byrne's (1984) view of play as a commonly-recognized-as-framed event in which the metacommunicative function always retains primacy. Consideration of the genesis of play in the child leads to doubt concerning the assumption that the identification of play is unproblematic because everyone knows play as framed. Freud's interpretation of the fantasy play of 18-month-old Ernst Halberstadt is a case in point, since it required familiarity with other features of the child's circumstances which could not be known by observation only. Quite likely it is necessary to take account of the individual child's reactional biography to provide an adequate description of apparently spontaneous play. "Framing," too, may have its genesis in reactional biography and may not withstand close study. Matthews and Matthews are correct to say that no scientific definiton of play exists. That does not mean it is not worth attempting to produce one. Deciding whether to abandon "play" as a candidate scientific term will ultimately require considerably more facts about playing and the identification of play than those presently known. (RH)

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THE IDENTIFICATION OF PLAY

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THE IDENTIFICATION OF PLAY

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Ullmann and Krasner's A psychological approach to abnormal behavior (1975), is worthy of attention not only by those interested in "mental illness". One of its great merits is the stress it lays on the extent to which an understanding of abnormal behaviour requires us to pay attention to how people come to be labelled mentally ill as well as studying the supposedly abnormal behaviour itself. It is our contention that the same holds true for play. A growth in the understanding of play will arise from asking questions about how play and non-play are labelled and what assumptions lie behind those labels, as well as studying the events and behaviours to which the word "play" refers. We wish to focus attention on the nature of those influences which lead the observer of behaviour to make the statement "This is play".

This view contrasts markedly with that advocated by Matthews and Matthews (1982) who propose a way of apparently avoiding this field of investigation. Matthews and Matthews' paper is essentially prescriptive, arguing against certain practices in the study of play and for another. When Matthews and Matthews argue that much definition in psychology is only spuriously scientific, we consider they are on firm ground. However they move from there to the categorical assertion that the place where definition belongs is "after the experimentation and observation, and not before it" (p 28). Furthermore they put forward as their distinctive contribution to the methodology of play study, the proposal that preliminary definition be replaced by a "paradigm case" approach. The illustrative example involves identifying cases of observed fantasy play and then checking the reliability of judgement between naive observers who have been asked to identify instances of fantasy play. The reliability obtained is taken to support the natural language use of "fantasy play" in research without the need for formal definition.

The procedure followed has certain attractions (see Vandenberg, 1982), but we are particulary worried by the prescriptive way it is presented, as well as by the Matthews' failure to match their criticism of others with an equally stringent self-criticism. Sutton-Smith and Kelly-Byrne (1984, p 306) have already pointed out that the Matthews' method "can sample only the values of the culture-bound judges". To this objection we would add the complaint that the data base used for the study was very restricted (same-sex pairs of four year olds in a laboratory playroom) and the reliability of the natural language term "fantasy-play" would need to be explored across more varied

settings and subjects. Furthermore, the fact that the authors are inviting other play researchers to follow them, in part on the basis of the supposed lesson from the history of psychology, leads us to raise another aspect of the history of psychology. Despite their use of a distinctive label "paradigm case approach", we note certain similarities between what they advocate and what has actually been the practice in another field of psychology. There is a tendency in the study of intelligence for some researchers to rely heavily on the intelligence test as a tool. The approach adopted to such tests often involves a playing down of the question of what intelligence is, in favour of a supposedly hard-nosed no-nonsense approach, "intelligence is what intelligence tests measure". (See Vernon, 1960, for a discussion of this "operational approach".) Correlation studies of their reliability and validity are offered as proof of scientific respectability of the tests. Without denying this approach certain successes, we would suggest that insofar as we understand intellectual behaviour better today than we did eighty years ago, credit does not rest solely with those who adopted a test-first, theorize afterwards approach.

In contrast to Matthews and Matthews, Sutton-Smith and Kelly-Byrne (1984) are, at least initially, more exploratory than prescriptive; certain notions of play are examined, their underlying socio-cultural assumptions are stressed, and their inadequacy for defining play are shown. Amongst the notions of play found wanting are play as voluntary activity, play as positive affect, play as egalitarian, play as flexible. As the paper unfolds, it seems that the conclusion might be similar to a point of view put forward some time ago by Berlyne (1969, p 843):

> ".. it looks as if psychologists would do well to give up the category of play in favor of both wider and narrower categories of behaviour."

However Sutton-Smith and Kelly-Byrne do offer an alternative notion to those rejected, albeit tentatively. They claim that

> "... play is <u>always a "framed" event</u> and everyone concerned knows that it is the case... In addition to its framed character (which is true of all other communicated realms), <u>in play the metacommunicative function always retains</u> primacy" (Sutton-Smith & Kelly-Byrne, 1984, p 317)

We find this approach encouraging, not because we consider that definition acceptable in its own right, but because it would appear to encourage play research to develop along lines of which we approve. Indeed it may strengthen the links between the study of play per se and what might be termed the study of the study of play. In the latter, one sort of question of central importance is: how do researchers identify and label behaviour as "play"?

If, as Sutton-Smith and Kelly-Byrne argue, the fact that play is "framed" is play's primary characteristic, then the nature of that framing becomes a central question in the study of play. Any activity may in certain circumstances be play, according to Sutton-Smith and Kelly-Byrne. What makes it "play" is that it is "framed" in certain ways. Just what these ways are seems to us worthy of careful attention. How is play identified by players and by observers? And do they always agree that a given action is "framed"?

There is one aspect of Sutton-Smith and Kelly-Bryne's approach which we wish to question, that is when they claim that "everyone concerned knows" that play is a framed event. This seems to imply that the identification of play is somehow unproblematic, a view apparently shared with Matthews and Matthews. This we doubt and our doubts are particularly strong when one considers the genesis of play in the child. Matthews and Matthews (1982, p 26) envisage even naive observers being able to say "That is a case of fantasy play if anything is!"

Consider the following case of eighteen month old Ernst Halberstadt:

> "The child had a wooden reel with a piece of string tied round it ... What he did was to hold the reel by the string and very skilfully throw it over the edge of his curtained cot, so that it disappeared into it, at the same time uttering (an) expressive "o-o-o-o". He then pulled the reel out of the cot again by the string and hailed its reappearance with joyful "da" ("there")." (Freud, 1961, p 9)

Would this necessarily be immediately seen as fantasy play by any naive observer? We doubt it. Yet Freud is able to make out a reasonable case for doing so. It was more than a fleeting observation, as he lived in the same house as the child for a number of weeks. Ernst was on good terms with his parents and conscientiously obeyed orders not to touch certain things. "Above all" Freud writes "he never cried out when his mother left him ..." although he was "greatly attached to his mother." Ernst had an "occasional disturbing habit of taking any small objects he could get hold of and throwing them away from him ... As he did this he gave vent to a loud, long-drawn-out "o-o-o-o",

accompanied by an expression of interest and satisfaction." Freud's interpretation of the "wooden reel" activity involved linking it to the child's lack of protest at the leaving of his well-loved mother and his satisfaction in throwing objects away. Ernst compensated himself for the mothers' leaving by staging the disappearance and return of the any object such as the reel. The throwing away of other objects was seen by Freud as the re-enactment of the first part only (disappearance) satisfaction being due to the fact that "gone" was a necessary preliminary to "return". (Freud, 1961, pp 8-10; also see Jones, 1957.)

The interpretation of the wooden reel incident as a fantasy game was possible because Freud was familiar with other features of the child's circumstances, features which could not be known by observing the incident in isolation. The incident made sense to Freud in the context of his observations of earlier activities. We are not arguing that Freud is necessarily correct in his interpretation. His case is a plausible one, however. It arises only because of knowledge of other aspects of the child's brief biography than the "play" itself, and we would suggest that any rival interpretation would require also to take account of that biography. In an earlier paper we tried to show the advantages of an interbehavioural approach to the study of childhood play (Cornwell and Hobbs, 1984). We stressed the need to take account of the psychological history (reactional biography) of the child in order to provide an adequate description of incidents of apparently spontaneous play, such as pretending to lift a "bird" from the wallpaper. We consider it potentially fruitful to study framing in this light too. Just as pretending to lift bird-substitutes has its genesis in reactional biography, so too may "framing". We would stress however that we are agnostic, sceptical even, as to whether Sutton-Smith and Kelly-Byrne's "framing" notion will stand up to examination. For if we look at its early development, will we be able to find, in the framing of play, characteristics which distinguish it from other phenomena? Freud's identification of the game required prior knowledge of the child's domestic circumstances. But it also rested on the interpretation of certain verbal communications. The interpretation of these signals or messages is open to question. As Sutton-Smith and Kelly-Byrne note, the criteria used to identify play are elusive.

Matthews and Matthews are correct to say that there is no scientific definition of play. That does not mean it is not worth attempting to produce

one. The process of putting it to the test may or may not lead us to eventually abandon "play" as a candidate for the status of a scientific term. But if ever a consensus on that is reached, it will probably be because we have learned a good deal more than we know now about playing and about the identification of play.

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